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Solid Earth
Discussions

Interactive comment on "Towards the application of Stokes flow equations to structural restoration simulations" *by* Melchior Schuh-Senlis et al.

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We thank the reviewers F.Maerten and P.Lovely for the time invested in the review, as well as their positive commentaries and constructive feedback. We also thank B.Kaus and A.Ismail-Zadeh for taking the time to read and comment on the manuscript. We apologize for overlooking some of their previous work, which shed another light on the novelty of the work done. The manuscript was modified according to those reviews. In particular, in order to go further with the method and add novelty to the work, the results of simulations on a new model including faults and a non-flat free surface were added and discussed in the manuscript. Here follow answers to the questions of the reviewers (for an easier reading, the reviewer comments are set in italics, and the answers follow in normal script).

Reviewer 1 (Frantz Maerten):

The article presents in a clear concise manner a new way of doing structural restoration using Stokes flow equations. The manuscript is well written and reads smoothly. The use of Stokes flow is clearly justified by the authors in the light of the geomechanical restoration problems (e.g., the non-physical constraint of flattening) but also when considering the difficulty of restoring structures with salt intrusions. I like publications that are based on simple ideas (here, the reverse time scheme used by the authors): Everything should be made as simple as possible, but not simpler. I think that the authors are paving the way for new ideas and developments in the domain of structural restoration, and we clearly see the potential for restoring more and more complex models, not only in 2D but also in 3D.

* *Even if faults are not yet included in the modeling, I do not see potential problems as the authors already deal with salt intrusion (interface between the rocks and the salt body). A specific viscosity for the faults can be used for the modeling, which was stated by the authors. So my first question is why the authors did not present a (synthetic) model with at least one faults, as all the ingredients are already here (coding)?*

After considering the reviews and commentaries on the manuscript, in order to add more value and explain further the possible applications of the restoration idea, another simple example of a model containing two faults and a free surface was added and discussed in the manuscript.

* *My second question (and suggestion) is related to rock properties, especially the poisson's ratio and the Young modulus. Is there a way to incorporate those properties in the process of restoration using Stoke flow equations? I think that this problem should be a little bit discuss by the authors as they can have an impact on the restoration process.*

Incorporating elasticity in viscous flow has been done, for example by using an effective viscosity to account for the elastic part of the material while minimizing the modifications to the viscous flow code (e.g. Moresi et al., 2003). The problem is that those schemes, like every implementation of elasticity, use values of the stress and strain at previous time steps. This poses the problem of the stress state at the begining of the simulation, on which the elastic behaviour part depends completely and which is not available in restoration schemes. However, specific material properties could still be taken into account in other ways in the restoration process. For example, the incompressibility constraint, which implies a Poisson's ratio of 0.5, can be relaxed (e.g. Thieulot, 2011 for the relaxation of the incompressibility), which could be used to account for lesser values of the Poisson's ratio. This discussion has also been added to the paper, in the discussion section.

* *Another suggestion is to provide information about the computation time of the models (or at least for some of them).*

The computation time was not added since the point is not set on code efficiency, and the code has not been parallelized, but it can be done.

Reviewer 1 (Frantz Maerten):

"Towards the application of Stokes flow equations to structural restoration simulations" presents a novel approach to structural restoration based upon principles of Stokes flow and deformation of Newtonian viscous fluids. The manuscript is well written and organized. The authors clearly explain the new approach and its implementation, provide clear and sound justification for the scientific principles, and demonstrate its potential value with three simple synthetic examples. While the current implementation and demonstration is limited to 2D, the potential extension to 3D is made clear. The manuscript is clearly worthy of publication, but I would first provide several comments and recommendations.

* *First off, it is my opinion that the authors do not adequately address their assumption of a linear (Newtonian) viscosity model in sections 1 2. The authors explain at some length in the introduction the limitations of elastic geomechanical restoration*

techniques to capture inelastic (nonlinear) processes. They also provide references to justify the representation of rock deformation as viscous flow. However, there is only brief mention in the discussion (line 297) of their simplification to assume Newtonian fluids. At the least, this assumption, and that most of the preferred representations of rock deformation as viscous fluids assume non-Newtonian (e.g. power law) models, needs additional (and earlier) acknowledgement and discussion. The first two examples of the new restoration technique use forward models that also assume Newtonian fluids. These are insightful; however, ideally, I would like to see a restoration of a forward model that uses more realistic rheology for the forward model.

Additional acknowledgement and discussion of the simplification introduced by (linear) Newtonian viscosity has been added to the introduction. The restoration of a more realistic model (coming from the numerisation of an analogical model) is on course but needs further work and will be published in a following article.

* *Second, I believe that the third example (Section 4.3) requires additional explanation and discussion. 1. It's not clear how the geometry was constructed. To what extent is this "image" an interpretation of real data vs. based on a model? How was it generated? The general reader should not have to read the reference to understand this model which is critical to this manuscript. 2. Also, why use a stochastically generated diapir rather than a previously published interpretation of a real structure? As a geologist, I would be more comfortable with an example that used a real subsurface structure than a stochastic model.*

The introduction of this model has been changed to be more thorough, as it was indeed not clear that the model actually comes from a seismic image. The model itself, as an interpretation of the seismic image, was generated through a stochastic code to determine the best position of the salt-sediment interface.

* *Further, the results of this section are very interesting, and probably warrant additional discussion. 1. It makes sense that the system tends toward a state that is in*

mechanical equilibrium (thus a flat salt-sediment interface). It would be nice to know that the restoration path is valid, too. 2. I'm having trouble understanding what are the geologic implications of the restored images in which synkinematic sediments are not removed. What is or is not representative of past state? What parts of the restored images should I focus on (and what should I not focus on)? There are significant differences between the models in the shallower section, but perhaps the authors do not discuss because they consider it geologically irrelevant. 3. It is important to note that the loading of shallower (younger) sediment is not removed and thus the stress state driving restoration in the past is incorrect. 4. A video (or several key frames) of the preferred restoration as it progresses back in time might add value in addition to showing only the final state of each.

Additional discussion of the results was added for this model to answer your answer. To summarize some key-points: the sediment and salt layers couldn't be restored to a completely flat state as the stress state inside the model are indeed incorrect for various reasons. A notable point of this result is that this salt diapir example is the result of upbuilding and not downbuilding. A video of the prefered restoration was also added, its link is available at the end of the conclusion, in the "video supplement" part.

* *The authors discuss the ability of this method to discuss faulted structures, and it seems the numerical implementation is ready. It would be nice to include an example.*

After considering the reviews and commentaries on the manuscript, in order to add more value and explain further the possible applications of the restoration idea, another simple example of a model containing two faults and a free surface was added and discussed in the manuscript.

* *The discussion of the numerical implementation (Section 3) is lengthy, and this detracts from the focus on structural restoration. Further, there are many prior implementations of Stokes flow using particle-in-cell methods. I recognize that the numerical implementation was much of the effort, but consider if it would be appropriate to con-*

dense this section and move the details to the Appendices (along with the validation examples). This could provide space in the manuscript for additional examples and discussion.

The presented implementation is indeed far from being the first of its kind, but those implementations are not always presented precisely, if at all. In this context, we feel that a clear presentation may help other authors. Additionally, the adaptive grid refinement part of the code is quite specific and has a great impact on the results that could be obtained. In this light, we chose to keep the discussion on the numerical implementation.

* *Finally, following are a few more technically specific comments. Figure 1: Verify that the velocity fields (BC) correspond to this sketch (A). I think that these velocity fields represent a single wavelength perturbation of the material contrast in the horizontal dimension, but the sketch shows two wavelengths perturbation. In other words, for this sketch, there should be four convection cells, not two, and material should be flowing up at the side boundaries in the forward sense.*

There was indeed a mistake in this figure, it has been corrected in the revised version of the manuscript.

* *Paragraph beginning line 279: Use of the term "weld" in the sense of restoration is confusing. The diapir is restored, and sediment is juxtaposed against sediment where there was originally no salt. This is not a weld in the geologic sense. To avoid confusion, I would recommend finding an alternate description of this feature of restoration.*

The term could indeed be misleading, and we chose to replace it with "salt scar".

* *Paragraph beginning line 321: The authors provide two solutions to the rock-air (or -water) interface problem: sticky air or the free surface. They go on to explain the issues with a free surface in some detail, but do not offer further discussion of the sticky-air solution. If it is a viable solution, why not demonstrate it?*

The sticky air method was still being implemented in the code at the publishing of the manuscript. Since then, the implementation showed that the method didn't stabilize the observed instabilities. This point has been added in the revised version of the article.

* *Reference to Medwedeff., et al.2016 (abstract) is now available in peer reviewed paper(Lovely, Jayr Medwedeff, AAPG Bulletin, 2018)*

The reference has been updated in the manuscript and bibliography.

* *Lines 65-67: I don't understand why large deformation and potential remeshing may limit the value for interpretation validation. Would remeshing not be OK, so long as key structural elements (e.g. faults and horizons) are preserved?*

Indeed, this part of the introduction was not clear and has been updated in the manuscript.

* *Line 111: Should a reference be provided for CFL condition?*

A reference to the original paper presenting the CFL condition was added.

* *Lines 136-139: Another reason not to solve the thermal equations is that diffusion maybe important at geologic time scales, and it is not reversible.*

Good point, which was added to the manuscript there.

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